

Geomorphic-based Evaluation of Potential Climate-Change Related Impacts on Sediment Storage, Lower Sacramento River

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Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0293: Geomorphic-based Evaluation of Potential Climate-Change Related Impacts on Sediment Storage, Lower Sacramento River

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

Summary: The proposal addresses an important topic of floodplain sediment budgets for the lower Sacramento River, the potential impacts of climate change on floodplain sediment storage, and the role that these floodplain sediments may play in downstream response to climate change. The proposal is strong in terms of documenting current and historic floodplain conditions (spatial volumes and ages of sediment), but weak in terms of developing a sediment budget that mechanistically describes channel-floodplain interactions and likely responses to climate change. Tasks 1-4 (documenting floodplain characteristics) have a high probability of success and would provide valuable information for subsequent geomorphic studies (this portion of the proposal has a rating of 2, above average/very good). However, the success of Task 5 (geomorphic response to climate change) is uncertain due to insufficient development of methods and unclear reliance on other sediment budgets and sediment transport models (resulting in a low overall rating). The technical reviewer ratings of this proposal were excellent, good, and good; however, the first review was discarded because it was non-critical and lacked substance. The remaining reviews agree with the rating given here (adequate). **Goals:** Clear and timely, but overstated in terms of developing a sediment budget and quantitative

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assessments of geomorphic response to climate change. Justification: Need for floodplain sediment budget well justified, but details of sediment budget model are lacking, making it unclear how authors' hypothesis will be tested (i.e., that floodplain sediment may contribute a significant volume of sediment to downstream reaches under future climate change scenarios). Approach: Methods for Tasks 1-4 are generally sound, and results will be of use to resource managers and future researchers conducting sediment budgets and assessing potential geomorphic response to natural/anthropogenic disturbances. Some concern that topographic data won't be accurate enough to confidently detect historic changes in floodplain volume (magnitude of elevation change and accuracy of data not reported in Fig. 4). Also, the sampling design for sediment cores is poorly explained/justified in terms of understanding geomorphic processes and future response potential to natural/anthropogenic disturbance. i.e., how should the floodplain be sampled to best elucidate the geomorphic processes and questions posed? Task 5 is not well developed (sediment budget will rely on unspecified methods for estimating input and output developed by others; mechanistic model for channel-floodplain exchange not specified in sufficient detail for evaluation; hydraulic model of sediment transport focused on in-channel flows with unclear relationship to floodplain erosion/exchange; Shields criterion may not be relevant for determining supply of floodplain sediment produced during floods). Feasibility: Tasks 1-4 will likely be successful, given the approach and the investigators' experience. However, the success of Task 5 is uncertain, as discussed above. Monitoring: Not applicable. Products: Documentation of floodplain sediments (volume, age, characteristics) will be important information for other sediment budgets and future geomorphic studies. Data files, methods, analyses, and scientific findings will be disseminated through a variety of outlets. Capabilities: Highly qualified for Tasks 1-4 of proposed work. PIs are familiar with the study area and issues, have performed similar analyses in the past, and will likely complete the work in an efficient, timely manner. Budget: Budget is appropriate and reasonable for the time frame of the project,

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although one reviewer questioned some costs (e.g., technical typing and indirect charges from UC Davis for some consultant tasks).

Additional Comments:

Summary: The proposal addresses an important topic of floodplain sediment budgets for the lower Sacramento River, the potential impacts of climate change on floodplain sediment storage, and the role that these floodplain sediments may play in downstream response to climate change. The proposal is strong in terms of documenting current and historic floodplain conditions (spatial volumes and ages of sediment), but weak in terms of developing a sediment budget that mechanistically describes channel-floodplain interactions and likely responses to climate change. Tasks 1-4 (documenting floodplain characteristics) have a high probability of success and would provide valuable information for subsequent geomorphic studies (this portion of the proposal has a rating of 2, above average/very good). However, the success of Task 5 (geomorphic response to climate change) is uncertain due to insufficient development of methods and unclear reliance on other sediment budgets and sediment transport models (resulting in a low overall rating). The technical reviewer ratings of this proposal were excellent, good, and good; however, the first review was discarded because it was non-critical and lacked substance. The remaining reviews agree with the rating given here (adequate). **Goals:** Clear and timely, but overstated in terms of developing a sediment budget and quantitative assessments of geomorphic response to climate change. **Justification:** Need for floodplain sediment budget well justified, but details of sediment budget model are lacking, making it unclear how authors' hypothesis will be tested (i.e., that floodplain sediment may contribute a significant volume of sediment to downstream reaches under future climate change scenarios). **Approach:** Methods for Tasks 1-4 are generally sound, and results will be of use to resource managers and future researchers conducting sediment budgets and assessing potential geomorphic response to natural/anthropogenic disturbances. Some concern that

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topographic data won't be accurate enough to confidently detect historic changes in floodplain volume (magnitude of elevation change and accuracy of data not reported in Fig. 4). Also, the sampling design for sediment cores is poorly explained/justified in terms of understanding geomorphic processes and future response potential to natural/anthropogenic disturbance. i.e., how should the floodplain be sampled to best elucidate the geomorphic processes and questions posed? Task 5 is not well developed (sediment budget will rely on unspecified methods for estimating input and output developed by others; mechanistic model for channel-floodplain exchange not specified in sufficient detail for evaluation; hydraulic model of sediment transport focused on in-channel flows with unclear relationship to floodplain erosion/exchange; Shields criterion may not be relevant for determining supply of floodplain sediment produced during floods). Feasibility: Tasks 1-4 will likely be successful, given the approach and the investigators' experience. However, the success of Task 5 is uncertain, as discussed above. Monitoring: Not applicable. Products: Documentation of floodplain sediments (volume, age, characteristics) will be important information for other sediment budgets and future geomorphic studies. Data files, methods, analyses, and scientific findings will be disseminated through a variety of outlets. Capabilities: Highly qualified for Tasks 1-4 of proposed work. PIs are familiar with the study area and issues, have performed similar analyses in the past, and will likely complete the work in an efficient, timely manner. Budget: Budget is appropriate and reasonable for the time frame of the project, although one reviewer questioned some costs (e.g., technical typing and indirect charges from UC Davis for some consultant tasks).

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The panel agreed that this proposal addresses important questions, but there were significant concerns regarding how the proposed research will quantify the geomorphic response to

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climate change. This portion of the proposal (Task 5) did not have sufficiently developed methods, and did not contain sufficient detail regarding how the proposed research will use other sediment budgets and sediment transport models. Therefore, the scientific value of this portion of the proposed research is uncertain. The proposal was strong in its documentation of current sediment storage on the floodplain and of the properties of that sediment.

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Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The goals, objectives and hypotheses are stated but are not always internally consistent with or supported by proposed methods. For example, on page 1 the authors state "the overall goal of our research is to develop a sediment budget model that includes the critical sediment storage component for the lower Sacramento River...." Much of the proposal is devoted to quantifying sediment storage, but development of an overall sentiment budget model, cited here as the overall goal of the research, is mentioned only briefly (page 15, Task 5) and is not listed as a project deliverable under any of the tasks. The authors also indicate that they will investigate "the volume and character of stored sediment that may be mobilized under various climate change scenarios" (page 1), although explanation of how this will be accomplished is extremely vague. The authors promise elsewhere in their introductory material that their research will document "the role of stored sediment to the overall sediment budget for the river, and... potential impacts of projected climate related river flows on sediment storage and transport" (page 3), but the proposal does not adequately explain how these items will be documented. The authors also indicate that they will "evaluate how past
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	climate induced variations in extreme runoff may have impacted sediment storage" (page 3) but this is not explained further. Project goals and objectives related more specifically to quantifying current sediment storage are more clearly stated. The overall goal of quantifying sediment storage and attempting to place this in a framework of future flows/climate changes is timely and important.
Rating	good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

Comments	The proposed research is justified relative to existing knowledge in the sense that improved understanding of sediment storage, which is a rarely quantified component of sediment budgets, is critical to understanding sediment routing, residence times, and budgets in the Sacramento / Bay-Delta system. The authors provide a conceptual model explaining the basis for the proposed work, including previous work by Gilbert and others related to sediment dynamics in the Sacramento River system. But, the justification of the proposed research in relation to future climate change (i.e., that it will provide insight into potential geomorphic effects of future climate change) is less convincing. The authors suggest that hydrologic changes associated with future climate change could alter the timing and magnitude of future floods in the Sacramento River, which could in turn alter sediment transport. The scientific basis of these propositions is difficult to judge, however, because they are supported only by references to an abstract and a paper in preparation, both of which include Florsheim as a co-author; are these propositions based on qualitative estimates, modeling,
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	or some other methods? Moreover, it is questionable whether existing knowledge of either the potential hydrologic or hydraulic effects of climate change, or of the mechanics of sediment mobilization from long-term storage reservoirs, is sufficient for meaningful analysis of future changes in sediment mobilization to be achieved.
Rating	good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	Overall, the methods proposed in Tasks 1-4 appear adequate for developing new information on sediment storage in the study area, including documentation of volumes of overall stored sediment. The methodology for achieving other project objectives is lacking in important respects, however. The introduction suggests that data on sedimentation rates will be derived here. Other than a brief reference to using radiocarbon dating to date cored sediments, further discussion of analysis and interpretation methods for determining sedimentation rates is not provided. Differentiation between pre- and post-mining sediment layers will presumably be used to provide coarse estimates of recent sedimentation rates (pre-mining vs post-mining); but will not allow differentiation of events like Shasta Dam closure. More cutting-edge dating methods, such as using short-lived radionuclides (e.g., Lead-210, Cesium-137) to date cored sentiments are not proposed but could be a
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useful addition for gaining additional resolution on the timing of post-mining sediment deposition. Methods of calculating historic post-mining sediment discussed on page 14 (Task 4) are unclear. The authors refer to employing tools such as LIDAR and SRTM data. If LIDAR data are available for the study reach, using such data to analyze current topography in relation to historic topographic data would be merited, but no details on this line of analysis are given. SRTM data are likely of insufficient resolution for this task.

The main flaws in the approach proposed here pertain to Task 5, in which the authors suggest methods for linking sediment storage dynamics to future climate change. The methods proposed here would not adequately address the working hypothesis on page 7 or the project objectives related to documenting the volume that "may be mobilized under various climate change scenarios." The methodology for estimating future changes in storage is vague at best and does not appear feasible, given the state of understanding about the mechanics of sediment mobilization from storage reservoirs and ability to predict the future, climate-related hydraulic changes that would govern such sediment mobilization, particularly in a system as highly regulated as the Sacramento River.

Task 5 proposes construction of sediment budgets for 3 time periods, including under future climate change scenarios. No explanation of how future sediment budgets will be constructed is provided, however, and the climate change scenarios (e.g., 2*CO2) to be modeled are not explained. To construct a future sediment budget, estimates not only of

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changes in storage, but also of sediment supply from upstream and of sediment yield to the Delta are needed. The methodology for estimating future changes in storage is questionable, and no plan is provided for how climate-related changes in input and output components of the sediment budget would be modeled for future climate scenarios? The authors appear to promise too much here by stating that they will construct sediment budgets for future climate change scenarios. Perhaps this is implicitly acknowledged in the failure to list results of sediment budgeting as a deliverable for Task 5.

No information is provided on where sediment coring will be focused (within active channel, along exposed bars, on the historic floodplain outside of human-made levees). The proposal implies, however, that coring will primarily be completed on the historic floodplain and that the focus of the proposed research is on understanding sediment storage along the floodplain as a whole. Although this would provide useful basic insights into sediment dynamics in the lower Sacramento River system, the relevance of these data to modeling geomorphic effects of climate change is less clear. Hydrologic changes associated with climate change would be expected to have the greatest effect on mobilization and transport of sediment stored in the channel bed and banks, rather than on sediment stored in longer-term floodplain sediment reservoirs. Furthermore, basic understanding of the mechanics of sediment mobilization from floodplains during high flows is inadequate, and no convincing methodology for identifying sediments that would be mobilized with increased flows is provided.

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	<p>The authors propose to investigate a distinct reach of the lower Sacramento River. There is no explanation, however, of why this reach was chosen, why it is representative or otherwise important, and what implications can be drawn from this reach for the broader Sacramento system and Bay-Delta system in general. Basic information about the study reach (length, area, levee characteristics) is not provided. The authors suggest that the data they will collect will be useful for sediment budgeting efforts, but inclusion of additional information about the study area would elucidate how these data are applicable to the broader Sacramento / Bay-Delta system.</p>
Rating	fair

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
 Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	<p>The elements of the proposed work related to quantifying sediment storage in the study area are feasible, well documented, within the grasp of the authors, and have a strong likelihood of achieving project objectives. The project team has completed sediment coring and interpretation along the Sacramento River and much of the work proposed here appears to be an expansion of this previous work to a new reach of the river. As discussed above, however, the feasibility of producing quantitative analysis of the potential geomorphic effects of climate change based on this work is uncertain, and the research approach related to this question is not adequately documented. A more feasible and yet still useful objective would be to use the information derived here on sediment storage, in conjunction with Florsheim's previous work, to derive a conceptual model of potential geomorphic effects of climate change in the</p>
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	lower Sacramento River system. A brief reference is made to the use of space shuttle ("STRM" sic; the proper abbreviation is SRTM) data (page 14), which does not seem feasible for this project given its resolution.
Rating	good

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	No monitoring is proposed here.
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	Project deliverables for each task are clearly delineated, and products of value are likely from the project, especially in relation to improved understanding of sediment storage in the lower Sacramento River system. These products will include compilations of relevant existing data, sediment coring data, interpretive geologic cross sections, isopach maps of stored sediment thickness, and GIS map layers illustrating data coverage, geomorphic characteristics, and sediment storage data. The authors also appear to have given considerable thought to dissemination of their results to both technical and non-technical audiences, including via peer-reviewed journals, conferences, workshops, and formal and/or informal presentations. Their work will contribute to larger data management systems, including the California Environmental Resources and
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	Evaluation System (CERES).
Rating	excellent

Additional Comments

Comments	This proposal is well written and well organized, with clear explanations of relevance to Calfed goals, tasks, deliverables, and personnel.
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Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The project team, consisting of Dr. Joan Florsheim and private consultants (from William Lettis Associates) is capable and qualified to perform the proposed research in an efficient manner. Florsheim has strong experience in the areas of sediment storage, sediment budgeting, Bay-Delta water issues, and potential climate impacts. Consultants from WLA have prior experience with sediment coring and interpretation and GIS analysis along the Sacramento River; their knowledge of the proposed study area and methods will likely facilitate efficient completion of their tasks. WLA also appears to have much of the infrastructure (e.g., coring equipment) needed.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	Overall the budget seems reasonable and adequate for the work proposed, which spans two years. Most of the requested funding is for salaries for Florsheim, a graduate student, and private consultants, plus additional funding related to data collection, map
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	preparation, and indirect costs. The billing rates for the private consultants involved in the project are high, and some tasks (e.g., "technical typing") could likely be completed for less by non-consultant personnel. Efficiencies related to the consultants' previous experience with tasks proposed here may offset their high billing rates to some extent. It is unclear when UC Davis should charge indirect costs for tasks performed by consultants not affiliated with the University (e.g., Task 1). As noted above however, the overall budget is appropriate.
Rating	excellent

Overall

Provide a brief explanation of your summary rating.

Comments	<p>The research proposed in Tasks 1-4, all of which relate to gathering and interpreting data on sediment storage characteristics along a reach of the lower Sacramento River, represents a useful project in its own right—the work is justified in relation to existing knowledge, addresses an important and poorly understood aspect of geomorphology (sediment storage) on a river (the Sacramento) with important controls on a sensitive and important ecosystem (the Bay Delta), and is addressed using a well designed and feasible approach by experienced authors. If the research proposal had been confined to these tasks (plus subsequent tasks related to information dissemination), it would merit a higher overall rating than is given here. But it is this reviewer's belief that the proposed work, especially Task 5, would be inadequate for addressing the project goals, objectives, and hypotheses related to the theme of climate change, as captured in the project title ("Geomorphic-based Evaluation of Potential Climate-Change Related Impacts on Sediment Storage, Lower Sacramento River") and in the working hypothesis (page 7). The methods related to assessing climate</p>
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	change impacts are inadequately explained, making their feasibility unclear, and the likelihood that the proposed work will produce valuable insights related to climate-change impacts is low in my opinion. In summary, as a project to improve basic understanding of sediment dynamics in the lower Sacramento River, the proposed research is worthwhile and valuable. But the proposal oversells the likelihood that the research will produce valuable understanding of future climate change impacts.
Rating	good

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proposal title: Geomorphic–based Evaluation of Potential Climate–Change Related Impacts on Sediment Storage, Lower Sacramento River

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	Goals clear but somewhat overstated. Determining the amount of sediment stored in Sacramento R floodplains a useful contribution. Little plausible connection made to sediment erosion and climate change.
Rating	very good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

Comments	Need for knowledge of stored sediment clearly justified. Strong experience and model given for measuring stored sediment. No conceptual model given for determining role of floodplains as a sediment source.
Rating	good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

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Comments	Field and analysis methods appropriate for determining age, location, and thickness of sediment deposits - which is the main product the work will provide. Three problems: (i) Claim of developing sediment budget (p. 15) questionable: it relies on estimates of inputs and outputs from others and these will be highly uncertain. (ii) Authors propose to determine volume of historic post-mining sediment by comparison of digital elevation data from vintage 5-ft contour maps with available DEM. I am skeptical that this comparison will have sufficient accuracy to provide usable information. (iii) No approach given for determining mobility of sediment. Unlikely that Shields Criterion is particularly relevant to the supply of floodplain sediment to a large river during flood. Calculation of transport capacity to be from existing hydraulic information and (presumably) some transport model. Accuracy of these calculations will be poor. How will this be accounted for in determining the volume of "potentially mobile stored sediment". What do you mean by "potentially mobile stored sediment"? Over what time frame? With what uncertainty? Useful for decision makers? Only indirectly. To quote proposal "The primary product of this research is quantification of sediment storage for use by regional modelers".
Rating	good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	Approach for estimating "potentially mobile stored sediment" not really specified. Project very likely to provide its basic information: an estimate of sediment stored in the Sacramento R floodplains. That is useful, important information.
Rating	good

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Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	Information on location, volume, and nature of floodplain sediment likely to be provided. This is an important piece of larger sediment budget. Lead PI likely to provide good interpretation of the results.
Rating	very good

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	All 3 primary investigators highly qualified and particularly experienced in the necessary disciplines.
Rating	excellent

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Budget

Is the budget reasonable and adequate for the work proposed?

Comments	Looks reasonable
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	<p>The bottom-line product of the proposed work: "quantification of sediment storage for use by regional modelers" is useful and an important piece for understanding the composition and adjustments of floodplains in Central Valley. I am confident that the authors will provide this. Unfortunately, they over-promise in terms of providing information on potential changes in sediment supply in response to climate change. The approach for that part of the work is hardly discussed and its prospects are far less promising because that kind of estimate would be very hard to make. Determining the volume and nature of the stored sediment is a start in that direction.</p> <p>I would like to give this a 3.5. Excellent for the sediment storage information it will provide and only fair in its claims to discuss potential sediment supply in response to climate change.</p>
Rating	good

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Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The goals, objectives and hypotheses are clearly defined and extremely well presented. Given the high likelihood of future changes in hydrological regime in the study area, I consider this project very timely and appropriate.
Rating	excellent

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

Comments	Most stream sediment budget studies do not consider sediment that has accumulated for centuries to millennia in proximity to river channels as a potential sediment source. Because climate change is likely to induce remobilization of stored sediments, it is of paramount importance to develop sediment budgets that include a sediment storage component. The volume of sediment that could be remobilized as a result of varying timing and magnitude of runoff under climate change scenarios could be significant. Increases in sediment supply to the Bay-Delta would have a deleterious impact on ecosystems and will most likely affect water operations and water quality (e.g.
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	through remobilization of mercury and other toxic metals from ancient sediments).
Rating	excellent

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	The study has a significant field based component (e.g. mapping and coring), which I consider a big plus. In addition, the PIs will compile data from historical and current maps, aerial photographs, and borehole data as well as use a suite of other sources of geomorphic information. This approach should allow them to successfully estimate the spatial distribution, character, and volume of sediment stored along river channels in the study area and develop a sediment budget model for the area that incorporates sediment storage.
Rating	excellent

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	This is a very ambitious project given its short duration but the PIs have previous experience with a number of the proposed research tasks and have ready access to the data sources necessary to successfully complete these task. Also, all the equipment needed for the field-based portion of the study is available to the PIs.
Rating	

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	very good
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Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	not applicable
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The study will provide valuable insight into the past geomorphic responses of the lower Sacramento System to anthropogenic activities and natural variations. This information can be used to predict the response of the area to future hydrologic changes and help refine regional sediment budgets and hydrological models. Deliverables will include a variety of digital topographic maps and GIS maps, compilations of survey data and historical records, and geologic cross sections indicating type of stored sediment and its thickness.
Rating	excellent

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

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Comments	The PIs have been previously funded by CALFED for a study that will significantly benefit (i.e. jump start) the proposed research. Their track records are excellent.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	The budget appears to be reasonable considering the scale of the project and the number of personnel involved. Note: The amount listed for Task 7, Office/Presentation Supplies should read \$240 instead of \$2400.
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	A very well written proposal with clearly stated goals and approach. I anticipate that the results generated by the study will be beneficial to both the scientific community and decision makers.
Rating	excellent

